A Cross-National Test of the Linkage Between Economic Inequality and Political Violence

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This study brings together for empirical testing three theoretical perspectives on the relationship between economic inequality and political violence. Using newly-available cross-national data on personal income inequality, we discover a moderate linear relationship between inequality and violence; neither curvilinear perspective on the inequality-violence linkage (the "V-curve" and the "inverse V-curve" hypotheses) receives even moderate support. Introducing four additional explanatory variables—relating to affluence, heterogeneity, social mobility, and rate of social change—does nothing whatsoever to bolster the explanatory power of income inequality.

A remarkably diverse literature, both ancient and modern, both ideological and theoretical, has coalesced on the proposition that political violence is a function of economic inequality. But despite the persistence of this theme, the extent to which and the manner in which inequality is linked to violence remain quite uncertain. Many different theories have been offered, but comparative studies are still scarce, with empirical findings scattered and anything but cumulative. Still to be brought together for testing in the cross-national literature are hypotheses drawn from a number of different theoretical perspectives. As yet unexamined, too, is the impact on violence of personal income in-

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equality, which would seem to have greater destabilizing potential than do the aspects of inequality treated in the existing research literature. The primary obstacle to cross-national research on income inequality has been the absence of personal income data, but such data have recently become available for a broad range of nations. The purpose of this study is to apply the newly available inequality data to a reassessment of the linkage between national levels of economic inequality and political violence.

THEORETICAL PERSPECTIVES

The idea that political violence is a function of the manner in which economic wealth and its concomitants, social status and political power, are distributed has been approached from a number of diverse perspectives. Aristotle considered inequality to be the "universal and chief cause" of revolutions, contending in *The Politics* that "Inferiors revolt in order that they may be equal, and equals that they may be superior." Centuries later, Madison in *The Federalist No. 10* characterized inequality in the distribution of property as the "most common and durable" source of political faction. Still later, Engels argued that political violence results when political structures are not synchronized with socioeconomic conditions. Even Coser (1957), whose theoretical work has emphasized the positive functions of social conflict, contends that conflict is largely the product of persisting socioeconomic inequalities. According to this diverse literature, antisystem frustrations are apt to be high where a substantial portion of the public does not share fully in the allocation of scarce resources. The present study focuses on the manner in which the distribution of one particular economic resource, personal income, affects the likelihood of political violence. Our initial hypothesis is simple and direct.

H1: The greater the inequality in the national distribution of personal incomes, the greater the level of political violence.

It has sometimes been argued, however, that the relationship between inequality and political violence is more complex than depicted in H1. Drawing on Aristotle's theory of revolution, Davis (1948) contended that a "stable economy"—by which he meant one that avoided either extreme concentration or extreme dispersion of incomes—would

discourage political violence.[1] A narrow concentration of incomes, Davis believed, would spur mass resentment and lead to revolution, while a wide dispersion would endanger elite dissatisfaction and ultimately cause civil war. Davis, a statistician, was not content to offer a vague formulation of the inequality-violence relationship, and therefore cast his discussion in terms of an operational indicator of income inequality, the Gini index. "Political disturbances," Davis hypothesized, would be most likely in nations where the Gini ratio exceeded a critical value greater than .5, or fell below a critical value less than .5. But Davis admitted that he had no idea of what these critical values might be, and accordingly treated the few economies for which he possessed income inequality data in terms of the magnitude of their deviation from the midpoint (.5) of theoretical maximum (1.0) and minimum (0.0) values of the Gini index. Our initial test of Davis' interpretation follows this precedent.

> H2a: The greater the national deviation in either direction from the midpoint (.5) of the income inequality scale, the greater the level of political violence.

To be sure, .5 is the midpoint of the scale of attainable Gini index values, and it is not at all unusual to find nations whose Gini value far exceeds .5 with respect to the distribution of *land;* in fact, the mean Gini value for the 54 nations on which Taylor and Hudson (1972: 267-268) report land inequality data is .67. But national Gini ratios for personal income inequality exceed .5 rather infrequently; of the 56 nations contained in Paukert's (1973) compilation, only 17 had personal income Ginis above .5, and the highest of these was only .64. To some extent, the lower Ginis for personal income than for land may simply reflect a tendency for the former to be more equally distributed than the latter, although we suspect that the lower magnitude of personal income Ginis is also something of a statistical artifact.[2] In any event, .5 may not be the appropriate point from which to gauge deviations in either direction, because although it is the midpoint of attainable values, .5 is unlikely to be the midpoint of an empirical distribution of scores. As an alterna-

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1. Davis' formulation seems to have been brought to the attention of a wide audience of social scientists by Kort (1952). The page numbers cited below are from the second edition of the Davis volume, published in 1954.

2. Gini was designed to summarize continuous distributions, but because of the crudeness of national income statistics must often be computed from discrete data containing only a few income categories. As Ray and Singer (1973) demonstrate, this can impose some fairly low ceilings on the values Gini takes on.

tive to H2a, then, we also offer a second, slightly revised version of Davis' thesis.

> H2b: The greater the national deviation in either direction from the mean of the distribution of national income inequality scores, the greater the level of political violence.

In effect, then, Davis depicted the inequality-violence relationship as a V-curve centered at .5 (or, as in H2b, at the mean) of the inequality scale, hypothesizing that violence is more likely in nations closer to the extremes of income concentration or dispersion. More recently, Nagel (1974) has offered another curvilinear interpretation of the inequality-violence relationship, but his argument is diametrically opposed to that of Davis. For Nagel, the key to political violence lies in relative deprivation, the notion that "objective" inequality affects behavior through subjective comparisons. "One individual," Nagel (1974: 454) writes, "measures his lot against another's, finds himself relatively advantaged or disadvantaged, and accordingly feels gratified or dissatisfied." The discontent triggered by inequality is said to be compounded from (1) the tendency of individuals to compare wealth (a tendency Nagel believes is inversely related to the amount of objective inequality), and (2) the extent of the grievance resulting from such comparisons (a direct function of inequality). Combining these two factors multiplicatively, Nagel suggests that the inequality-violence relationship resembles an *inverse* V-curve, with political violence most likely at intermediate levels of ine   'ity and least likely at the extremes of concentration and disper.

> H3: The less extremely concentrated or dispersed the national distribution of personal incomes, the greater the level of political violence.

We have, then, broad theoretical agreement that economic inequality begets political violence, but three entirely different perspectives on the nature of the linkage—the traditional straight-line interpretation, Davis' simple V-curve, and Nagel's inverse V-curve. In addition, although it is not our purpose to test a comprehensive theory of political violence, we must also consider the possibility that certain other national characteristics may operate in such a way as to obscure the true nature of the relationship between inequality and violence.

It seems possible, for example, that the political implications of inequality may vary dramatically from impoverished to affluent

nations. Low absolute levels of wealth could aggravate the frustrations engendered by inequality, while affluence might offset these frustrations. That is, the likelihood of violence may depend not only on the manner in which wealth is distributed, but also on the amount of wealth available for distribution.[3]

A second possible mediating influence is the rate of social mobility. The impoverished masses in a highly stratified system may be less frustrated if there is a meaningful chance for them to improve their lot within the foreseeable future. Alternatively, rapid mobility might prove to be profoundly destabilizing if a socioeconomic elite, perceiving that its position is in jeopardy, takes preemptive action to defend itself. In either case, the operative force propelling violence would be the combined impact of inequality and mobility rather than the independent operation of either.[4]

Like affluence and social mobility, the extent of sociocultural heterogeneity present within a system must also be considered, because, in the first place, "discontinuity, cleavages, dissociation and strain, deriving from cultural discrepancies are the hallmark of plural society" (Lofchie, 1968: 13). Beyond its direct impact, sociocultural heterogeneity may, much in the fashion of low absolute levels of national wealth, aggravate the frustrations induced by inequality; or, in a far different manner, *if* it cuts across rather than reinforces the economic stratification system, sociocultural heterogeneity may actually moderate the destabilizing impact of inequality. Although these potential mediating effects work in opposite directions, in either event bringing sociocultural heterogeneity into the analysis could do much to clarify the nature of the inequality-violence relationship.[5]

Finally, the rate of social change a system is undergoing may also mediate between inequality and violence. Even a fairly equalitarian distribution of incomes could be linked to high levels of political violence in a system experiencing the profoundly unsettling process of rapid change.[6]

Of course, any number of other factors could conceivably mediate between inequality and political violence. But we feel that in affluence,

3. The relationship between level of affluence and political violence has been addressed in literally scores of previous studies; see, e.g., Feierabend, Feierabend, and Nesvold (1969).
4. For some interesting theoretical speculations on the mobility-violence relationship, see Gurr (1968).
5. On the relationship between sociocultural heterogeneity and political violence, see, e.g., Cooper (1974).
6. The best statement concerning the impact of rapid social change on political violence and instability remains Olson (1963).

social mobility, sociocultural heterogeneity, and rate of social change we have isolated the four most likely candidates for factors which could, if their operation were left uncontrolled, obscure the relationship with which we are centrally concerned, that between personal income inequality and political violence.

RESEARCH PERSPECTIVES

The pioneering cross-national research on the inequality-violence relationship was Russett's (1964) study of the political impact of inequality in land tenure systems. Although he expected to find strong relationships between national levels of land inequality and political instability, Russett could document only low to moderate correlations between several indicators of each in 47 nations. Probing the possibility that these relationships were being suppressed by outside factors, Russett included the Gini index of land inequality as one predictor in a multivariate model of political instability. His report of this phase of the analysis is quite sketchy, but entering per capita gross national product, percentage of the labor force in agriculture, and land tenancy into the analysis apparently did enhance the predictive power of land inequality. But even these multiple predictors left more than half the variance unexplained, and the impact of land inequality remained rather insubstantial. Accordingly, Russett was forced to conclude that "The old saws about equality can be accepted only with caution."

A subsequent study by Parvin (1973) provides even less support for the idea that political violence is a function of economic inequality. Working with a sample of 26 predominantly Western nations, Parvin regressed a measure of political unrest (deaths from domestic group violence per million population) on a set of economic determinants that included per capita income, socioeconomic mobility, urbanization, rate of economic development, communications intensity, and intersectoral income inequality. These factors together accounted for an impressive portion of the variance in political unrest ($R^2 = .67$), but the contribution of intersectoral income inequality proved to be only marginally significant and—even more troublesome—inverse. That is, in the Parvin study political unrest was found to occur with somewhat greater frequency in more egalitarian societies, controlling for the effects of the remaining factors. Parvin could not adequately account for this unanticipated finding. He did emphasize, however, that per capita income, a measure of absolute economic well-being, was a far more influential factor than income inequality, a measure of relative well-being.

The more recent research reported by Nagel (1974) does not entirely clarify matters. Although his analysis of 26 Vietnamese provinces produced results which were consistent with his curvilinear interpretation (see H3 above), Nagel's attempt to replicate these findings over a sample of 54 nations was far less successful. Regressing seven different indicators of political violence on per capita gross national product, rate of economic development, the Gini index of land inequality, and the Gini index squared (the nonlinear term), Nagel achieved consistently poor results. In no case, he found, were the coefficients of both the Gini index and its square statistically significant. Subsequently, Nagel (1976) has reported that flaws in his original statistical analysis caused the impact of land inequality to be underestimated in some cases, but these flaws were hardly of sufficient magnitude to occasion a fundamental reconsideration of his earlier conclusions.

In sum, the recurrent stipulation of a strong relationship (of whatever form) between economic inequality and political violence has not been supported by previous cross-national research.[7] Still, we feel that this relationship has yet to be satisfactorily tested. Land inequality, the focus of the best empirical work, is not an adequate cross-national indicator of economic inequality, because in many societies—particularly in those at higher developmental levels—life chances are not closely tied to land ownership.[8] Intersectoral income inequality, despite the frequency with which it has been employed in cross-national research, is at best a second-rate measurement proxy for personal income inequality, lacking theoretical interest of its own.[9] Accordingly, the

7. One other research study did turn up evidence indicating that successful revolutions were somewhat more likely to occur in nations marked by high levels of land inequality, but this finding was only a very minor part of a comprehensive treatment of the causes of revolution (Tanter and Midlarsky, 1967).

8. This point has also been made by Russett (1964).

9. Lacking data on the distribution of personal income, some researchers (e.g., Jackman, 1975; Cutright, 1967; Parvin, 1973) have attempted to approximate personal income data with an index of intersectoral inequality devised by economist Kuznets. The intersectoral measure compares the equality of product per worker in eight national economic sectors. Since the bulk of any sector's product is composed of workers' incomes, Kuznets reasoned that sectoral product per worker would approximate sectoral income per worker. Hence, where the proportion of the labor force in each sector equals the proportion of the product of each sector, intersectoral inequality equals zero.

Despite the ingenuity of this index, its usefulness to researchers who are interested in personal income inequality is open to serious question. The index does not consider differences *within* sectors, so that within the agricultural sector the wealthiest *hacendado* is lumped indiscriminantly with the humblest *peon*. Nor have intersectoral differences themselves been the focus of a great deal of social science theorizing. For the 32 nations for which data are now available on both intersectoral and personal income inequality, the Pearsonian correlation between the two is .69—substantial, but hardly of a magnitude that would warrant using intersectoral data to test hypotheses about personal inequality.

primary message we would extract from the existing research literature is not that political violence is necessarily unrelated to economic inequality, but rather that the relationship needs to be investigated with a more apt index of economic inequality.

MEASUREMENT CONSIDERATIONS

INDICATORS

Personal Income Inequality. Until recently, economists had assembled very little comparative information on personal income inequality, perhaps due to Pareto's insistence that the distribution of income is fixed, remaining constant irrespective of place or time (Paukert, 1973; Tarascio, 1973). As a result, the compilers of a recent cross-national data collection were forced to conclude that "The most salient difficulty facing the scholar who wants to compare inequality of income over several national units . . . is the absence of sufficient data" (Taylor and Hudson, 1972: 212). But the outlook for cross-national analysis began to brighten with the appearance of a set of personal income distribution data for 44 nations, collected by Adelman and Morris (1973). Paukert (1973) subsequently upgraded the quality of these data by dropping four nations for which Adelman and Morris' basic information was wholly inadequate, replacing their data for three nations, recalculating data for nine nations, and adding data for 16 additional countries. The result was a refined compilation of data on the distribution of personal incomes in 56 nations, as summarized by the Gini coefficient.

The Gini index derived from Paukert's personal income data was adopted as our basic measure of income inequality. Davis, however, contended that it is not the extent of inequality per se, but rather the extent to which the pattern of distribution deviates from the "stable economy," that leads to political violence. Accordingly, we calculated the absolute difference between .5, representing Davis' notion of economic stability, and each nation's Gini index, and used this measure in testing H2a. Our procedure with regard to H2b was similar, except that each nation's Gini was subtracted from the arithmetic mean of national Gini scores (.448) rather than .5.

Finally, in order to test H3, we followed Nagel's (1974) lead and calculated the square of the Gini index, so that it could be inserted along

with the Gini coefficient itself in a regression equation. Of course, this procedure introduced substantial multicollinearity, due to the monotonic relationship between the Gini index and its square. But, as Nagel (1974: 462) argues, while multicollinearity does produce large standard errors in estimates of regression coefficients, the significance of a coefficient is inversely proportional to its standard error. Thus, if coefficients are highly significant despite large standard errors, regression estimates should be accepted despite high intercorrelation between the regressands.

Political Violence. The primary problem we faced in measuring political violence was less one of locating suitable data than of choosing an apt measure from among the rich variety of candidates. After considerable thought, we decided to employ a measure devised by Hibbs (1973) for his sophisticated comparative analysis of the causes of mass political violence, in the hope that using the Hibbs measure would enhance comparability with other cross-national studies. Hibbs factor-analyzed data on six types of mass antisystem events of political significance occurring between 1958 and 1967. One of the two principal factors proved to be composed of a set of indicators of what Hibbs called "internal war." Hibbs constructed a summary measure of internal war by computing the natural log of the sum of the three indicators loading highly on this factor—assassinations, armed attacks, and deaths from domestic violence[10]—and it was this summary measure which was adopted here.

Affluence. Again, numerous possibilities, all of them closely linked, presented themselves. Here, we followed the traditional practice of employing 1965 per capita gross national product (logged) as our indicator of national affluence (Taylor and Hudson, 1972).

Social Mobility. Measuring the openness of society's opportunity structure is always problematic in comparative research, in that quantitative data on rates of social mobility are available for only a handful of highly advanced nations. In this situation, we decided to operationalize social mobility in terms of the breadth of national educational enrollments. Our reasoning was that because educational level is a key to improvements in social status, a narrow educational base would

10. We chose not to employ the other summary measure devised by Hibbs, "collective protest," because although it is a useful index of the extent of organized mass protest activity, we do not deem it an apt indicator of political violence as such.

severely limit the potential afforded by society for individual and group advancement—an argument that has been advanced by previous researchers faced with the same measurement problems (Adelman and Morris, 1967; Parvin, 1973). The educational measure used here, the adjusted school enrollment ratio (1965), summarizes primary and secondary enrollments as a percentage of the population of primary and secondary age, adjusted to correspond to the actual duration of schooling (Taylor and Hudson, 1972).

Sociocultural Heterogeneity. As with political violence and level of affluence, the cross-national literature contains several candidate indicators of sociocultural heterogeneity, all of which are closely interrelated. Primarily because it was available for several more nations than any other existing indicator, we chose to employ a fractionalization index computed by Taylor and Hudson (1972) from ethnolinguistic data presented in the Russian *Atlas Narodov Mira.* The *Atlas* makes little distinction between ethnic and linguistic differences, determining groups not by physical characteristics but by roles, descent, and relationships to others; as Taylor and Hudson note, the Russian data thus index what amounts to ethnolinguistic assimilation. Taylor and Hudson, by computing an index of the type devised by Rae and Taylor (1970), created a measure of fractionalization from these data, an index which we adopted as our operationalization of sociocultural heterogeneity.

Rate of Social Change. In accordance with Lerner's (1958) contention that urbanization is the key component of the multidimensional process of social change, we operationalized rate of social change in terms of the difference between 1950 and 1960 percentages of population residing in cities of 100,000 or more (Taylor and Hudson, 1972). Rather than simply calculating percent change from the one period to the next, we followed a residualization procedure (Van Meter, 1974) which involved regressing the 1960 urbanization score on its 1950 counterpart and calculating the difference between the observed 1960 value and the value predicted from the highly accurate linear regression ($R^2 = .862$). A positive residual indicates that a nation was more urbanized in 1960 than would have been predicted on the basis of its 1960 score, and a negative residual carries the opposite meaning. The advantage of this procedure, of course, is that change scores are not simply artifacts of 1950 urbanization level, of which they are statistically independent.

Population Size. One additional variable, the natural log of 1965 population size, also had to be employed (Taylor and Hudson, 1972). In order to avoid the problems that often accompany the use of ratio variables containing a common term, Hibbs (1973) did not per-capitize the indicators from which he created his internal war index. He did, however, recognize that the magnitude of internal war might, ceteris paribus, be a partial function of population size, and accordingly included a population term (the natural log, which maximized the linear effect of population) in each regression equation in his study. We have followed Hibbs' precedent because we agree that doing so "allows a judgment about the impact of variables of central importance in the presence of the logically prior but theoretically uninteresting size variable" (Hibbs, 1973: 25).

SAMPLE

The sample for this study consisted of 49 nations, comprising almost all of those for which data are currently available on personal income inequality. Paukert's compilation of income inequality statistics presents data on 56 nations, but two of these, Fiji and Surinam, could not be matched for any of the other variables employed here. In addition, Hibbs did not include Barbados, Gabon, or Trinidad in his study, and Taylor and Hudson coded South Africa and Malaysia as missing for the enrollment ratio and 1950 level of urbanization, respectively, so these five nations also had to be dropped from consideration.

By the standards of cross-national aggregate data research, where sample sizes often exceed 100, a 49-nation sample is not especially large. Moreover, selecting a sample of nations on the basis of data availability raises the question of representativeness, in that data availability is known to be related to level of national development; in fact, some previous researchers (e.g., Hudson, 1967) have gone so far as suggesting that an index of national data availability could serve as a *measure* of societal modernity. How representative is our 49-nation sample of the population of nations from which it was drawn? In order to address this question, we first calculated sample and population means, standard deviations, and minimum and maximum values for those variables for which such comparisons could be drawn—population size, per capita gross national product, the enrollment ratio, sociocultural heterogeneity, rate of urbanization, and internal war. These comparisons, presented in Table 1, should allay most fears about the representa-

TABLE 1
Comparison of Sample Statistics and Population Parameters

Variable	n	Mean	Minimum	Maximum	s.d.
Population Size (log)					
Sample	49	9.3	7.2	13.1	1.4
Population	135	8.7	4.6	13.5	1.6
GNP/Capita (log)					
Sample	49	5.9	4.2	8.2	1.2
Population	135	5.7	3.6	8.2	1.2
Enrollment Ratio					
Sample	49	61.2	6.0	101.0	26.4
Population	135	57.8	5.0	105.0	27.1
Heterogeneity					
Sample	49	.37	.00	.93	.30
Ppulation	136	.39	.00	.93	.29
Urbanization					
Sample	49	0.0	−18.0	16.1	6.4
Population	129	0.5	−46.8	58.2	10.0
Internal War					
Sample	49	4.6	0.0	9.3	2.5
Population	108	4.4	0.0	11.2	2.9

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tiveness of the sample. In every case, without exception, there are only small mean differences between sample and population. Moreover, (with the possible exception of urbanization), comparing the minimum and maximum values and standard deviations reveals that the 49-nation sample contains virtually the full range of diversity found in the broader population.

Even more revealing is the sample-population comparison of correlations linking the six variables, presented in Table 2. At the very least, these comparisons suggest that the structure of relationships embedded in the sample is not grossly unrepresentative of those found within the universe of nations. In fact, in fully 12 of the 15 instances, sample correlations were virtually identical to those obtaining for the universe, diverging by less than .10. In sum, our 49-nation sample appeared to be quite similar to the population of nations in terms of the central tendencies and dispersions of those six variables, and very much the same sorts of relationships obtained in the sample as in the population. Although the sample was certainly not random and contined fewer than 40% of the world's polities, on the basis of this evidence we felt justified in concluding that it was essentially representative of the broader population of nations.

FINDINGS

Table 3 presents the results of a series of multiple regression analyses undertaken in order to test the three hypotheses specified earlier. Regression 1 reveals that the magnitude of national political violence is indeed related to population size, although this relationship is a moderate one ($R = .337$, $R^2 = .113$). More interesting, we see in Regression 2 that including the Gini index of personal income inequality along with the population size variable almost doubled the variance explained in internal war ($R^2 = .215$, adjusted $R^2 = .198$).[11] The fact that the Gini index had a statistically significant impact when it was entered alongside population size was taken as initial support for H1, although it should be borne in mind that the impact of personal income inequality appeared to be rather moderate.

Regressions 3, 4, and 5, however, provided virtually no empirical support for the second and third hypotheses, for in no case did a curvilinear interpretation improve on the explanatory power of the simple

11. "Adjusted R^2 is an R^2 statistic adjusted for the number of independent variables in the equation and the number of cases. It is a more conservative estimate of the percent of variance explained, especially when the sample size is small" (Nie et al., 1975: 358).

TABLE 2
Comparison of Correlations, Sample and Population*

	Lg. Pop.	Lg. GNP/Cap.	Enroll.	Hetero.	Urban.	Int. War
Lg. Pop.		.076 (49) .050 (135)	.136 (49) .051 (130)	.066 (49) .092 (136)	.055 (49) -.128 (129)	.337 (49) .291 (108)
Lg. GNP/Cap.			.818 (49) .796 (130)	-.620 (49) -.392 (135)	.227 (49) .215 (129)	-.400 (49) -.330 (108)
Enroll.				-.689 (49) -.442 (130)	.112 (49) -.146 (124)	-.298 (49) -.205 (107)
Hetero.					-.187 (49) -.118 (129)	.348 (49) .251 (108)
Urban.						.107 (49) .146 (108)

*The first entry in each cell is the Pearsonian correlation for the sample, followed in parentheses by the sample size; the second entry is the correlation for all nations for which data are available, along with the corresponding N.

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TABLE 3
Regression Summaries for H1 through H3

Statistic	Regression				
	1	2	3	4	5
Constant	-1.15	-6.61	-1.13	-1.63	4.36
Pop. (Ln.)	.616*	.753**	.671*	.600*	.735**
	(.251)	(.246)	(.257)	(.251)	(.243)
Gini		9.328*			-40.626
		(3.824)			(35.811)
$\mid .5 -$ Gini $\mid$			-6.225		
			(6.142)		
$\mid \overline{X} -$ Gini $\mid$				8.380	
				(7.537)	
Gini^2					55.607
					(39.641)
R	.337	.464	.364	.369	.498
R^2	.113	.215	.133	.137	.248
Adj. R^2	.113	.198	.114	.118	.215
F	6.01*	6.29**	3.52*	3.63*	4.94**

NOTE: The first entry for each predictor is the parameter estimate, b; the second (in parentheses) is the standard error of b.
*$p < .05$; **$p < .01$.

linear approach followed in Regression 2. The first version of the "stable economy" interpretation offered by Davis (Regression 3) fell flat on its face. Not only did the absolute difference between .5 and a nation's Gini index fail to add anything to the predictive power of sheer population size, there proved to be a *negative* relationship between political violence and the distance of the Gini coefficient from .5 (denoting Davis' stable economy). Too much should not be made of this inverse impact in light of its moderate magnitude, but it is clear that the findings summarized in Regression 3 were wholly inconsistent with the expectations of H2a.

H2b, the version of Davis' interpretation in which the focal variable was the deviation from the *mean* of national Gini ratios, fared only slightly better than H2a (Regression 4). To be sure, the direction of its impact was at least in the predicted direction, but the magnitude of this impact was very slight (R = .337 for population size alone versus .369 for the two-variable model). Nor, in conjunction with Davis' original speculations, were we successful in our exploratory attempts to discover any "critical values" of the Gini coefficient above or below which inequality was closely associated with political violence.

Regression 5, which tested H3 by including both the Gini index and its square, improved marginally on the predictive power of the simple linear interpretation tested in Regression 2 (R = .498 versus .464, adjusted R^2 = .215 versus .198). But here again, as with Regression 3, prediction should not be equated with explanatory power, for theoretical expectations were clearly violated in Regression 5. Not only did the (wildly inflated) coefficients of the Gini index and its square both fall short of statistical significance, but the signs of the two coefficients ran directly counter to Nagel's prediction. Whereas Nagel expected to find low levels of violence where inequality is either very low or very high and greater violence where inequality is moderate, Regression 5 summarizes what amounts to a weak version of the opposite, or V-curve relationship. Ironically, then, our test of Nagel's hypothesis produced a result closer to the one predicted by Davis than by Nagel, although, as with Regression 3, too much should not be made of this relationship because it was so very weak.

The regressions summarized in Table 3, then, provided some moderate empirical support for H1 and no support whatsoever for H2a, H2b, or H3. It still seemed possible, however, that some uncontrolled third factor or set of such factors was suppressing the inequality-violence linkage; if this were the case, incorporating such a factor in the analysis

would allow the true nature of the relationship between inequality and political violence to emerge. We were especially interested in probing two possibilities. First, would controlling for the effects of other relevant factors—namely, social mobility, sociocultural heterogeneity, rate of social change, and affluence—enhance the predictive power of personal income inequality? Second, would personal income inequality add anything to the predictive power of these other factors?

In order to answer these questions, we undertook several additional analyses in which the impacts of school enrollment, heterogeneity, urbanization, and per capita gross national product (taken both singly and in combination) were assessed along with the impacts of population size and the four personal income inequality terms. This undertaking obviously produced a great many regression equations—far more than can be presented here. Let it suffice to say that with respect to the curvilinear interpretations of the inequality-violence relationship, introducing the four potential suppressor variables did nothing to change the basic pattern of findings summarized in Regressions 3, 4, and 5 above. That is, Hypotheses 2a, 2b, and 3, which were essentially inaccurate when considered apart from social mobility, sociocultural heterogeneity, rate of social change, and affluence, remained just as inaccurate when considered in conjunction with these factors. Accordingly, the failure of these hypotheses could not be attributed to the operation of external factors—or at least to the four factors we considered the prime candidates to mediate between economic inequality and political violence.

With respect to H1 (the simple linear hypothesis), however, our probes for suppressor relationships are of greater interest. Table 4 summarizes the relevant analyses.[12] Considering the Gini index in conjunction with the adjusted school enrollment ratio (Regression 7) provided greater overall predictive power than obtained when either inequality or enrollment was taken separately (Regressions 2 and 6, respectively). Both the Gini index and the enrollment ratio had statistically significant impacts in Regression 7, combining with population size to produce a respectable R of .545. On the other hand, the anticipated suppressor effect did not emerge. The impact of the Gini coefficient was not boosted by the introduction of the school enrollment ratio; in fact, comparing the results of Regressions 2 and 7 reveals that

12. In the interest of conserving space, regressions involving pairwise combinations of enrollment, heterogeneity, urbanization, and gross national product per capita are not presented in Table 4. The results of these regressions were entirely consistent with those summarized in Table 4.

TABLE 4
Regression Summaries for Suppressor Effect Probes

Statistic	Regression								
	6	7	8	9	10	11	12	13	14
Constant	0.10	-4.52	-1.78	-6.92	-1.07	-6.56	3.76	-1.00	-1.35
Pop (Ln.)	.703** (.238)	.801* (.236)	.576* (.239)	.709** (.233)	.607* (.253)	.745** (.247)	.676** (.227)	.775** (.225)	.694** (.229)
Gini		7.572* (3.736)		8.840* (3.627)		9.410* (3.844)		7.212* (3.572)	7.877* (3.573)
Enrollment	-.034** (.013)	-.028* (.012)							.021 (.023)
Heterogeneity			2.727* (1.087)	2.591* (1.035)					1.791 (1.433)
Urbanization					.035 (.055)	.039 (.052)			.087 (.049)
GNP/Cap. (Ln.)							-.921** (.267)	-.820** (.263)	-1.016* (.459)
R	.483	.545	.469	.558	.348	.474	.544	.595	.642
R^2	.233	.298	.220	.311	.121	.224	.295	.354	.413
Adj. R^2	.217	.267	.203	.281	.102	.191	.280	.326	.344
F	7.00**	6.35**	6.49**	6.77**	3.17	4.34**	9.64**	8.22**	4.92**

NOTE: The first entry for each predictor is the parameter estimate, b; the second (in parentheses) is the standard error of b.
*$p < .05$; **$p < .01$; ***$p < .001$.

inequality had a more substantial impact alone than when considered along with enrollment. Conversely, income inequality did not contribute to greater predictive power on the part of the enrollment ratio; the parameter estimate for the enrollment ratio was actually higher in Regression 6 than in Regression 7.

Precisely the same pattern of findings emerged when ethnolinguistic heterogeneity was inserted into the analysis. As expected, heterogeneity proved to be significantly related to internal war (Regression 8), and considering heterogeneity jointly with income inequality produced the most substantial effect yet encountered (R = .558 for Regression 9). But, as with the enrollment ratio, there was no indication that being considered in conjunction with the heterogeneity term did anything to bolster the impact of income inequality (compare the statistics for the Gini index in Regressions 2 and 9), or that controlling for income inequality markedly affected the impact of ethnolinguistic heterogeneity (compare the statistics for heterogeneity in Regressions 8 and 9).

Nor did introducing the urbanization variable change matters appreciably. As can be seen in Regression 10, rate of urbanization possessed precious little independent predictive power, and urbanization and income inequality together (Regression 11) had no greater impact than income inequality alone (Regression 2). That is, the contribution of income inequality remained essentially unchanged irrespective of whether it was considered alongside urbanization or in isolation; and the contribution of urbanization remained paltry in any event.

Unlike urbanization, gross national product per capita proved to be closely linked to internal war. Although previous theorists and researchers (e.g., Feierabend, Feierabend, and Nesvold, 1969) have often posited a curvilinear relationship between affluence and political violence, the results of Regression 12 suggest a powerful linear impact. But adding personal income inequality into the mix (Regression 13) had rather disappointing results. R moved only from .544 in Regression 12 to .595 in Regression 13, with an associated increment in the adjusted R^2 statistic of only 4.6%. That is, although the impact of inequality remained marginally significant, affluence and inequality together did not explain a much greater share of the variance in political violence than did affluence alone. Moreover, leaving affluence uncontrolled appears if anything to have enhanced rather than suppressed the impact of inequality, as a comparison of the parameter estimates for the Gini index in Regressions 2 and 13 makes clear. And here again, as with the enrollment ratio, heterogeneity, and rate of urbanization, being con-

sidered in conjunction with income inequality did nothing to enhance the predictive power of gross national product per capita; in fact, the opposite proved to be the case.

In sum, Regressions 6 through 13 convinced us that the impact of income inequality on political violence was not being suppressed by the workings of an external variable. Analyzing all of the predictor variables simultaneously (Regression 14) reaffirmed this finding, for the effect of income inequality remained well below its simple counterpart in Regression 2. In light of the substantial coefficients for GNP per capita, we must conclude with Parvin that the overall level of societal well-being is a more critical determinant of political violence than is income inequality, a measure of relative well-being.

DISCUSSION

We began by examining three different theoretical perspectives, all of which agreed that political violence is strongly linked to economic inequality, but all of which disagreed on the form of the inequality-violence relationship. Two of these perspectives—Davis' V-curve (H2a and H2b) and Nagel's inverse V-curve (H3) interpretation—were wholly unsupported in our 49-nation analysis. There was moderate empirical support for the remaining perspective—the traditional linear interpretation (H1)—support that could not be bolstered by introducing additional predictors into the analysis. How can we account for this failure to provide strong empirical support for a relationship so often stipulated in political and social theory? Our answers to this question are necessarily speculative, but several seem worthy of consideration.

In the first place, we must realistically acknowledge the possibility that our findings reflect problems of measurement rather than theory. Despite the best efforts of several scholars (Adelman and Morris, 1973; Paukert, 1973; Chenery et al., 1974; Lydall, 1968), cross-national data on income distributions are still distressingly scarce and imprecise. For example, most of the inequality data employed here pertain to the early-mid-1960s, but in a few instances Paukert was forced to rely on data more than a decade distant. Then, too, the Gini indices were evidently computed from pretax income distributions and hence do not reflect redistributive policies. Measurement problems leave studies like the present one in something of an interpretative quandary. Do our findings reflect the true nature of the income inequality-political

violence relationship, or are they products of an inadequately measured variable? Until more refined data on personal income distributions become available, we simply cannot be certain.

We can, however, also hazard some substantively-oriented interpretations of our findings. Among other things, the cross-sectional character of this research must be borne in mind. We have probed the question of whether differences among nations in levels of personal income inequality are related to differences among nations in levels of political violence. Despite our conclusion that there is no substantial cross-sectional relationship, the *dynamics* of inequality may still prove to be highly predictive of the amount of political violence within nations over time. This interpretation, in fact, is close to the Marxian theory of revolution, which holds that it is the progressive degradation of the working class which foments violence. Of course, others have held sharply divergent notions, e.g., Tocqueville's idea that economic advances rather than declines trigger revolutions. But, whether one accepts Marx's or Tocqueville's formulation or some synthesis of the two (e.g., Davies, 1962), the clear implication is that it is historical changes in inequality patterns which account for political violence. Accordingly, it is possible that income inequality and political violence are in fact closely related—but over time within nations, rather than at a point in time across nations. Unfortunately, currently available data are woefully inadequate for testing such longitudinal interpretations.[13]

Data are also inadequate for testing another line of speculation. Structural conditions such as the level of income inequality may be only weakly related to citizen attitudes, beliefs, and values, for, as Dahl (1966) has argued, the causal chain connecting objective socioeconomic conditions with actual political behavior is long and tenuous. But it is attitudes, beliefs, and values (e.g., status frustration, alienation) rather than structural conditions per se that ultimately produce political violence. Moreover, societies would appear to differ to a surprising degree in the extent to which objective and subjective inequality correspond (see, e.g., Scase, 1973). Accordingly, an adequate explanatory model of the inequality-violence relationship would encompass not only objective measures of inequality but also subjective estimates of inequality and popular perceptions of the stratification system. Because

13. In his survey of evidence on national income distributions, Paukert (1973) presents longitudinal data for only Denmark, Germany, the Netherlands, Norway, Prussia, Saxony, Sweden, the United Kingdom, and the United States. Allais (1973) includes historical data on some other systems, even including ancient Greece, but these historical data are of highly suspect quality and, in any event, are not available for a large enough set of polities to permit systematic analysis.

cross-national survey research is still in its infancy, however, such models are likely to remain untested for the foreseeable future.

Finally, as Betz (1974) contends, "income inequality" may simply be too gross a concept to possess explanatory utility for social systems as complex as states and nations. The basis of Betz's argument is the fact that complex systems are apt to be divided along several lines of cleavage—cultural, religious, racial, ethnic, and linguistic, as well as economic. The key to understanding political violence, according to this interpretation, lies not in analyzing the impact of any single cleavage or even the additive impact of a set of cleavages, but rather in probing the *interactions* among the various lines of cleavage. Are these cleavages cross-cutting or cumulative? Even profound economic inequality may produce only minimal political effects *if* it cuts across other deep-seated social and cultural cleavages; in such a circumstance, the basis for effective group action would likely be absent. Where economic, social, and cultural cleavages converge, however, the potential for political violence should be greatly enhanced. Of course, we are again dealing in the realm of the speculative, for nothing approaching the highly refined data necessary to gauge the cumulative or cross-cutting nature of societal cleavages is currently available.[14]

We are admittedly uncertain about which of these interpretations best accounts for our findings. For the moment, we must be content to have called for greater refinements in measurement and model specification, and to have suggested that a time-honored idea of political and social thought—the notion that political violence is a product of economic inequality—may be in need of some rethinking.

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14. An interesting start in this direction has, however, been made by Rae and Taylor (1970).

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